

Peer-review report of

Lalot, F., & Abrams, D. (2025). Partisan forgiveness of political leadership in the 2024 UK general election: Are there limits to transgression credit? *advances.in/psychology*, 2, e972114. <https://doi.org/10.56296/aip00040>

Round 1

Dear Authors,

Thank you for submitting your important work to our special issue on the Psychology of Pushback. I was fortunate to receive evaluations by two experts in the field. I read and evaluated the manuscript myself before reading their evaluations. We all see strengths in the current work but also areas for improvement.

Reviewer 1 finds the study timely, well-written, transparent, and a worthwhile contribution, but offers a detailed checklist of improvements. Besides others, the reviewer asks for clearer theoretical grounding (a point reiterated by Reviewer 2), fuller election context, and more precise terminology. Methodologically, they recommend explaining why candidate-specific models were run, avoiding claims that require statistical tests (or alternatively, provide the latter), and clarifying measures. In the discussion they urge restraint in interpreting scale points, greater nuance about election-loser forgiveness, and a deeper limitations section that considers types and severity of betrayal, apologies, and cross-national perspectives.

Reviewer 2 is more critical but also judges the topic, literature review, and longitudinal data set as strong. They note that the manuscript lacks an integrated theoretical framework that is tested, which weakens its contribution. The reviewer calls for major re-thinking: derive concrete unanswered questions from the literature, test them with integrative models (e.g., ingroup vs. outgroup forgiveness moderated by party, identification, trust, and election outcome), and weave results back into theory and the Special-Issue theme on democratic pushback (both in Intro and Discussion).

Please address each of the reviewers' specific points in a revision. In addition, please address the following:

1. Your use of SEM with a residualized outcome is appropriate, yet omitting T2 predictors constrains interpretation. If T2 measures are available (please confirm), re-estimate the models as latent change-score (LCS) models that enter each variable at each time point. The analyses suggested by Reviewer 2 can supplement (though presented in main text)—rather than replace—the SEM to add nuance.
2. Avoid phrasing that presumes verification of the null hypothesis (e.g., “in contrast, party identification played no role in forgiveness of the ingroup leader”). Reviewer 1 raised the same concern.
3. Could the three models be visualized (showing the main stats, such as B, 95% CI, p values).

4. Please provide fit indices for the SEM.
5. Please make sure to present exact p -values for all tests, in main text and in tables.
6. Please avoid causal language when referring to variables at T1 being associated with the outcome at T2 (e.g., “National identification increased forgiveness of Sunak and Farage among outgroup voters”). The model does not allow for causal inferences like this.
7. Please describe the manipulation checks.
8. Please drop the subheading “Constraints on Generality” as this is already conveyed in the main heading.

In summary, the manuscript shows real promise, but substantial revisions are required to meet the standards of both the special issue and the journal. Please submit your revised version by 2. August. If you need more time, let me know as soon as possible. The typical word limit for this article type is 5,000 words, and your current draft is just under 4,000. Should you need additional space to address the reviewers’ points, a length of up to 6,000 words is acceptable.

Best,

Jonas R. Kunst

Editor-in-Chief

Reviewer 1:

I read this manuscript with great interest. It has many very positive qualities. The topic that it addresses – forgiveness of leader transgressions – is an interesting one with both theoretical and practical implications. The authors identify relevant literature, describing work concerning who, when, and why leaders may be forgiven or not. The review of the literature is nicely done – drawing on related findings and explaining them clearly. The dataset that the authors use, with its longitudinal design, is a particularly valuable and relevant resource. The results appear stimulating.

Nevertheless, I also have reservations about the current manuscript. Some of these might be addressed by some re-thinking, reframing, and rewriting of the current material. Others involve re-analysis, and the contribution of the work may depend on how the data align with theoretically-based expectations.

While the review of the literature suggests several potential effects, the text never comes together in a conceptually integrative way that synthesizes implications of the literature review and data analysis in a manner that seems to test or advance theory. No specific predictions are offered at the end of the Introduction. Instead, despite the impressive review of the literature that points to several concrete expectations, the work is mainly framed as broadly exploratory: “We investigated

absolute levels of forgiveness for each of these leaders as well as pre/post-election change. We also explored how much party identification (i.e. ingroup identity), country identification (superordinate identity), and levels of trust in each political leader, influence willingness to forgive post-election.” The case for a new significant theoretical contribution of the work is never convincingly developed, despite the presentation of a solid theoretical and empirical foundation of interesting findings that potentially could be woven into a strong rationale for the work.

Here is my dilemma. I don’t want to put the authors in a position of producing predictions post hoc, but I am not convinced that the work makes a sufficient theoretical or empirical contribution without some sort of integrative rationale.

Beyond my concern about the magnitude of theoretical contribution of the work, I worry a bit that the design is essentially quasi-experimental, which would prevent definitive cause-and-effect inferences. However, the design does not automatically prevent me from seeing the work as publishable, but it does make presenting a compelling theoretical case even more important.

So, in its current form, I cannot recommend the manuscript for publication.

Still, though, I believe that there is a kernel of a valuable study here, and I be willing to see a revised version of the manuscript. Given the nature of my current reservations, though, I could not predict the outcome.

My main suggestion is that, if the authors revise this work for this or another journal, they should relate the conclusions they draw from the literature review to specific analyses that would test the implications with the current dataset. Currently, while Table 1 and 2 present some interesting findings, the results seem to be treated in a piecemeal manner. There are a number of ways the authors could create a more integrative set of analyses and, to illustrate my point, I offer some examples.

- The authors describe in their literature review a core finding that people are generally more forgiving of ingroup leaders than outgroup leaders. How might this be tested with current data set? One way would be to examine ingroup-leader vs. outgroup leader forgiveness before the election. My pursual of the current data indicates there there is strong support for the idea that people are more forgiving of ingroup than outgroup leaders. While this is not surprising given the substantial findings of a range of ingroup vs. outgroup biases in the literature, the authors might further examine the robustness of the effect by testing whether this effect is moderated by voter political party: Conservative Party Members, Labour Party Members, and Reform Party Members – the three parties with leaders standing for election – that is a 2 (Ingroup vs. Outgroup Leader) x 3 (Political Party Affiliation) test.

- It is further suggested by the literature review that the ingroup-outgroup effect would be more pronounced among voters who identify more strongly with their party. (The authors label this variable Social Identity in the Method section, but I recommend labeling this consistently as Party Identification, as the authors refer to it Table 2). This variable could be added to the model proposed above to test for moderation by Party Identification. Interestingly, The results presented in Table 2

suggest that Ingroup Party Identification does not relate to Willingness to Forgive Ingroup Leaders but strong Identification with an Outgroup Party predicts less forgiveness of an outgroup leader.

- The authors might also consider National Identification in that model to test whether it also has a moderating influence but in the direction of reducing the ingroup-outgroup effect. The effect of Trust might be similarly tested. While Table 2 presents findings relevant to this and the previous point, I am suggesting that the authors test this in a more integrative way, looking at the ingroup-outgroup effect as moderated by Party ID, National, ID, Trust, and Voter Party.
- Then the authors might then focus on the change score might be used to test for the impact of election outcome. The authors discuss the importance of whether the actions of a group member/leader are beneficial or detrimental to a group on whether a leader may receive “transgression credit” or not. Winning an election is likely viewed a beneficial, and losing an election is likely seen as detrimental. This would suggest that the Labour Party participants, whose candidate won the election, would show a comparable level of forgiveness for their leader (Starmer) after the election compared to before the election or grant even more forgiveness after the election. For Conservative Party members, because their candidate lost the election, they would like offer less “transgression credit” to their leader (Sunak) after the election than before. I note, though, that the results do not seem to align with this reasoning. Conservative Party Members showed an increase in forgiveness for their leader after the election (.42) that was similar and even a bit greater than the increase that Labour Party members exhibited for their leader (.38). (Also, I am not sure about how to handle Reform Party members, because their candidate did not seem to have a realistic chance to win the election. Based on similar reasoning above, strength of party identification, national identification, and trust might also be tested as moderators of change.

Again, I offer these suggestions to illustrate how the literature review, data analysis, and discussion could be more tightly integrated. Better alignment of these elements could potentially strengthen the contribution of the work and make a stronger case for publication in this or another journal.

If the authors have the opportunity to resubmit a substantially revised version of the current work for the Special Issue in this journal, I also recommend that they more explicitly link the work to the theme of the Special Issue: The Psychology of Pushback: Understanding Resistance and Compliance During Democratic Decline. I do see relevance of the current study to the Special Issue topic, but the connection needs to be articulated more directly in the text in both the Introduction and Discussion. The current Discussion is largely a rehash of specific results in the present research. I believe it would be stronger if the authors presented a briefer summary of findings (which may be easier if the integration of the literature, its implications in this context, and the analyses was tighter, as I recommended earlier). That might allow the authors more opportunity to foreground at bigger-picture levels how the current work (1) aligns with previous findings and theory, and (2) relates and contributes to the theme of the special issue.

In conclusion, while the various concerns I have expressed about the manuscript prevent me from supporting publication of this version of the work or from recommend a formal “invited revision” the manuscript, I do see potential in the work. However, substantial rethinking, reanalysis, and rewriting would be needed to make the kind of contribution – to the research and to the focus of the Special Issue – that I believe the journal expects. Given the fundamental nature and magnitude of revision that would be required, I could not predict the outcome of a resubmission process. Nevertheless, I encourage the authors to revise the manuscript and, even if not for this journal, continue to pursue publication of the work. It is interesting, timely, and promising work.

Reviewer 2

I had the pleasure of reading this very interesting work on forgiveness toward political leaders. The authors present a longitudinal investigation conducted in the context of the 2024 UK General Election, measuring forgiveness before and after the election and examining the moderating roles of trust, party identification, and national identification. The manuscript is very well written and clearly adheres to open science standards. The topic is both timely and relevant, with implications that extend beyond the UK context. I also want to emphasize that using the same data set for multiple projects is not inherently problematic, provided that this is clearly and transparently communicated, as the authors have done. As such, I believe this work makes a valuable contribution to the field.

Below, I have included some suggestions for improving the clarity of the paper, as well as questions that the paper raised for me and that I would encourage the authors to address.

1) Theoretical Introduction

1.1) Overall, the authors introduce the key concepts clearly and present their theoretical reasoning convincingly. However, at certain points, some (side) aspects are mentioned without sufficient explanation. For example, on p. 2, the authors state that “people have a general tendency to be more lenient toward ingroup transgression” and that “the same applies to group leaders.” However, no citation is provided, and I wondered whether the analogy is truly that straightforward given that leaders are a very specific type of ingroup member, often associated with power and responsibility. It might be helpful to either cite a relevant source or briefly explain why this tendency should apply similarly to leaders or individuals in positions of power, as it does to regular group members. Also, on p. 3, the authors write that “when judging political leaders’ behaviour, citizens must weigh ethical concerns against loyalty to their preferred party.” To clarify this point, the authors might include a few examples of what kinds of ethical concerns they mean. Finally, on p. 5, the authors mention winner-loser dynamics as a possible factor influencing forgiveness but do not elaborate further. One option would be to explain more clearly what the authors expect in this regard. Alternatively, they might consider moving this point to the

discussion section, as it is only briefly mentioned at the very end of the introduction and seems only loosely connected to the other themes introduced earlier.

1.2) Minor: The authors raise the question of “how much” pre-existing levels of trust, etc., influence the willingness to forgive (e.g., p. 2, p. 5). In my view, the phrase “how much” suggests a comparison between different moderators, which does not appear to be the authors' intention. It might be clearer to instead ask, for example, “whether” trust influences willingness to forgive.

1.3) Minor: In the Present Research section, the authors might consider reporting the election results (percentages) for candidates other than Nigel Farage as well, just to provide readers with a more complete overview.

2) Methods and Results

2.1) If I understand correctly, the authors measured participants' willingness to forgive betrayal (p. 7). Against this backdrop, I am not entirely sure whether the term “transgressions,” used throughout the manuscript, is the most appropriate. While betrayal may indeed be a common type of political transgression in the UK, in other contexts political authorities may violate human rights, imprison or torture opponents, censor media, or shut down internet access. I mention these examples to illustrate that “transgression” is a very broad term. Since the focus of this study is specifically on forgiving betrayal, it may be clearer and more accurate to label it as such (e.g., on p. 12: “This research investigated voters' willingness to forgive political leaders for their past [transgressions] betrayal in the context of the 2024 UK General Election”).

2.2) The results section was generally very clear, and I have only a few questions. On p. 8, the authors state that “Forgiveness also aligned on a left-right party spectrum: Liberal Democrats were most similar to Labour voters, while Reform were most similar to Conservative voters.” Could the authors clarify how this alignment is (statistically) shown in the data? Or is it a descriptive observation? If it is the latter, it would be helpful to explicitly state that. Additionally, it was not completely clear what exactly this alignment means and why it is particularly noteworthy.

2.3) On p. 8, the authors state that “all voters showed a similar increase.” The notion of similarity seems tricky if it has not been tested (for instance, in equivalence testing). I would encourage the authors to refrain from using the term similarity (unless statistically tested) and rather stick with notions like “no significant differences observed.”

2.4) In general, I was also wondering why the authors decided to run separate pre-post analyses for different candidates rather than running one model (main effect: time, main effect: candidate, interaction: time*candidate). Maybe they could briefly explain their choice in the paper.

2.5) In Table 1, the final column is labeled “Effect of group.” Since the study does not include (experimental) groups, it may be clearer to use the phrasing “Differences depending on respondent's vote” (as in the table title) or “differences between voters of each party” (as in the text) or something along those lines.

2.6) Minor: On p. 8, when discussing the pre-post analyses, the authors might more explicitly remind readers that they tested whether the difference score significantly deviated from zero. This might help avoid confusion for readers (e.g., why the intercept is reported).

2.7) Minor: Did the authors adapt their social identity measure from an existing scale (e.g., Leach et al., 2008), or was it developed specifically for this study? A brief clarification would be helpful.

3) Discussion

3.1) At the beginning of the discussion (p. 12), the authors describe their findings in terms of “moderate forgiveness,” referencing the position of responses on the scale. However, it is quite difficult to draw meaningful conclusions about the level of forgiveness solely from scale point averages. There is ongoing debate about how to interpret effect sizes (e.g., Lakens and colleagues), and interpreting scale points seems even more ambiguous without additional justification.

3.2) On p. 12, the authors state that their findings suggest “that, at least initially, election losers are willing to ‘reach across the aisle.’” Do the authors refer here to voters of the losing party? As currently worded, the statement seems to suggest conclusions about the losing candidates themselves, although the data only provides insight into perceptions of voters. Additionally, in the same paragraph, the authors argue that “it is troubling that new incumbents might feel empowered to engage in transgressive or deviant actions...” This interpretation seems very broad and, in my view, cannot be drawn from the data. I would encourage the authors to remain closer to their empirical findings, which are interesting in their own right.

3.3) On pp. 12-13, the authors discuss the specificity of results for Nigel Farage, discussing these results against the backdrop of Farage’s status as a political outsider. This is a plausible and interesting interpretation. However, I would encourage a more nuanced view. For example, the authors mention that it is surprising that even an election loser (Sunak) was forgiven. Yet they have two losers, and only one was forgiven. One could argue that the Farage result is less surprising, and it is the Sunak result that deviates from expectations. A further question is in how far forgiveness is being extended to the individual (e.g., Sunak or Farage) vs. the party they represent or to the political offices they hold. Do the people forgive Sunak as a politician or Sunak as a Conservative Party figure, or Sunak as someone who served as Prime Minister for two years? These distinctions, though speculative, could help sharpen the interpretative framework.

3.4) Lastly, the current limitations section (pp. 14-15) focuses mostly on standard methodological points: sample size, representativeness, and measurement choices. While important, these caveats apply to virtually all psychological studies – the sample could always be larger or more representative and the measures could always be different. Particularly valuable, however, might be to also reflect on conceptual and theoretical limitations of the present work. For instance, the study examines betrayal very broadly. However, betrayal can take many forms (e.g., corruption, deception, abuse of power). Future research could examine in more

detail whether the type of betrayal matters for forgiveness. Additionally, does the severity of the betrayal matter (how many people were affected, or what were the consequences of the betrayal for the public)? Also, more typical perspectives from forgiveness literature might be considered. For instance, did the politician publicly apologize for the betrayal (and if so, how/under which circumstances, e.g., Hornsey et al., 2020)? Finally, international perspectives could be discussed more. The authors briefly touch on the point that the UK represents a large democratic country. An interesting question for future research might be whether the present findings would replicate in countries with a different democracy index than the UK. These are just a few limitations and directions for future research that I see emerging from the paper.

Overall, the paper presents an interesting and timely contribution, and it may serve as a generative contribution to the field!

Hornsey, M. J., Wohl, M. J. A., Harris, E. A., Okimoto, T. G., Thai, M., & Wenzel, M. (2020). Embodied remorse: Physical displays of remorse increase positive responses to public apologies, but have negligible effects on forgiveness. *Journal of Personality and Social Psychology*, 119(2), 367–389.
<https://doi.org/10.1037/pspi0000208>

Round 2

Dear Authors,

Thank you for submitting the revised version of your manuscript to our special issue. I appreciate your responsiveness to the reviewers' comments—the progress is clear.

Both reviewers agree that the manuscript has improved substantially, yet they also point out a few remaining concerns. Please address these in what I hope will be the final round of revisions. In terms of Reviewer 2, please make sure to provide the additional two-level ingroup/outgroup analysis as a form of robustness test.

In addition, kindly attend to the following minor points:

- Please provide 95% CIs also in the figure(s).
- Apologies for the misspelling in the previous decision letter; I was referring to the attention check, not manipulation check. Please describe it.
- The CLPM seems appropriate for two-wave data, but its main limitation (conflating between-group differences with within-person change) should be discussed.

- Please avoid contractions (e.g., “don’t”; I think there is only one instance in the manuscript).
- The abstract should more clearly state and be organized around its goal to test the two opposing hypotheses: transgression credit vs. black sheep effect.
- In Table 2, explain what estimates in parentheses refer to.
- Is it possible to graph some of the interactions observed in the first set of analyses? Otherwise, it becomes quite complex to rely on Table 2 to understand results. Please visualize with 95% CIs.
- Please always provide three digits for p-values (also when the last digit is zero; p values under .001 are correctly presented as <.001).
- There are a few minor discrepancies in citations that should be aligned with APA7. Please adjust the issues identified in this citation report (note that there are some false positives that you can ignore):
https://drive.google.com/file/d/1pcokzAns3MLKQWCdtG2SU5QUWDrw_igB/view?usp=sharing
- Please make sure there is a space before and after a *p*-value.

Thank you again for thoroughly revising your manuscript. I believe it is very close to the threshold warranting acceptance in our special issue. Please address the remaining issues within a month’s time, i.e., by September 15.

Best,

Jonas R. Kunst

Reviewer 1:

I was Reviewer 2 for the original version of the manuscript. While I had reservations about the original manuscript, I recognized the promise of the work and am happy to see the current revision. The authors appear to have taken the guidance offered by the Editor, Reviewer 1, and me seriously and made genuine (and effective) efforts to address them. The authors were generally responsive to the comments. As a consequence, the manuscript is much improved. I am even more convinced than before that this work could merit publication in the Special Issue. However, I see a few lingering issues and ways that the work can be further strengthened.

While I found the revised version of the manuscript clearer and better organized, I still believe that a stronger bridge from concepts to analyses is needed. The paragraph on page 9, lines 5-18 (beginning, “In this context, we measured British voters’....”) provides a nice overview of the main objectives of the work. However, the jump from concepts (e.g., “own leader”) to operationalizations and labels in the analyses (e.g., Vote in the Tables) is not explicit enough. This would also be a place to prepare readers for the analysis strategy of having 4 different levels of Leader, rather than the two-level (ingroup vs. outgroup) that is the conceptual partition the

Introduction primarily makes. These connections among concepts, operationalization, and analytic design decisions are important to make more explicitly.

Revising and expanding the narrative may not be sufficient, though. In the case of “own leader” and “vote” is not simply an issue of adding text about how intended vote (Vote) and “own leader” relate, but also there is some conceptual slippage that needs to be addressed (or at least acknowledged and justified). Specifically, a central concept in this manuscript is ingroup (vs. outgroup) categorization and identity. The implication in the Introduction is that the ingroup is based on party affiliation, which is more explicitly indicated in the description of the measure of ingroup (political party) identification. Previous work indicates that connection to a particular political leader (e.g., fusion) and party (ingroup) identity identification are not the same (see, for example, Kunst, Dovidio, & Thomsen, 2019). Perhaps this can be addressed narratively, but it may also indicate the need to involve some additional analysis with party affiliation operationalizing ingroup categorization.

I also continue to have reservations about the conclusions drawn by the authors of ingroup vs. outgroup leader effects based only on the results for the different leaders. For example, the authors write on p. 12, lines 17-19, “Decomposing these interactions revealed clear leniency for one’s own party (Table 2) without exception, voters of each party were much more willing to forgive their own leader for past trust violations than were other voters, both pre- and post-election.” However, in Table 2, for Starmer at T1 and T2, participants from the same party (Labour) did not significantly differ from LibDems. The authors do observe that Liberal Democrats (typically centre-left) gave ratings closer to Labour voters (left), while Reform (typically right-populist) gave ratings 2 closer to Conservative voters (right).” But that seems to be shifting ingroup categorization from political party to political ideology. I believe that it is important to align the ingroup-outgroup boundary that the authors believe is central and hold firmly to that in describing ingroup and outgroup differences in responses. Deviations of results from predictions relating to core categorization of ingroup and outgroups based on political party (vs. liberal vs. conservative ideology) can then be considered in the Discussion. In addition, as I suggested in my previous review, I think some analysis that directly tests the effect of the core ingroup-outgroup categorization should also be included rather than a narrative interpretation of the results for separate parties and candidates. I appreciate the authors’ point in their response letter that “each leader differs from the others on various dimensions,” and am fine with the authors presenting the 4-level variable as their main analysis. Those results are interesting and informative. However, if the authors want to conclude something about ingroup vs. outgroup differences in responses to leaders, it seems important to use that 2-level classification directly in an analysis. Perhaps that analysis could be presented in the main text as a supplementary analysis, noting that leaders differ on many dimensions, and thus framing that analysis as a kind of “robustness test.”

In conclusion, I commend the authors for their responsiveness to the previous round of comments and, as I mentioned earlier, I still see significant promise in the work. However, I believe that more revision is needed – mainly around articulating

clearly what the core basis of ingroup/outgroup categorization is assumed to be and then aligning operationalizations and interpretations with it –before I could support the work for publication.

Reviewer 2:

The authors did a great job responding to the reviewers' comments and suggestions. Their revisions to the introduction, especially regarding the competitive test of the transgression credit versus black sheep hypotheses, as well as their additional analyses and more thorough discussion of the implications and limitations, have substantially improved the manuscript. I congratulate the authors on their work and hope it inspires follow-up studies in other political contexts.

I have only two minor comments left:

1) Results: It would be helpful to provide readers with clearer guidance on what each test is theoretically intended to inform. For example, on page 12, the text could include a statement such as, "To test the transgression hypothesis versus the black sheep effect, we first examined differences between groups of voters..." When introducing the cross-lagged panel models on page 15, for instance, it would be useful to briefly mention what the longitudinal associations between forgiveness, trust, and identification reveal conceptually.

2) Table 1: The authors indicate in Table 1 that mixed model results are presented, but the actual linear mixed model estimates (e.g., `summary(mod1)` in R) are not shown. Instead, they present the ANOVA test showing which predictors significantly contribute to the model (`anova(mod1)`). This should be clarified (in the table header/text), including a brief note on how the model was specified (e.g., random intercepts only, no random slopes, etc.).